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I have examined the foregoing account and certify that it is correctly cast and properly vouched.

EMORY MCCLINTOCK,
Auditor.

June 23, 1902.

The chief feature of the closing session of the Association in the Music Hall of the Carnegie Institute on Thursday evening, July 3, was an illustrated lecture by Mr. Robert T. Hill on the recent volcanic eruptions in Martinique, in which the chief features of his recent investigations were described. After the lecture a series of resolutions were passed expressing the thanks of the Association to the various persons and organizations in Pittsburgh concerned in the organization of the meetings and entertainment of the members.

D. T. MACDOUGAL,
General Secretary, A. A. A. S.
NEW YORK, July 5, 1902.

APPLIED BOTANY, RETROSPECTIVE AND PROSPECTIVE.*

It has been the general practice in past years for the retiring Vice-President of this

* Vice-presidential address before the Section of Botany, American Association for the Advancement of Science. Pittsburgh meeting, June 28 to July 3, 1902.

Section to give a summary of the results accomplished in research work, and to point out the lines along which there appears promise of further advancement. The facts set forth in these addresses and the opportunities pointed out in them have proved of great advantage to all, especially the younger men, who draw their inspiration from what has been accomplished in the past and what the future holds forth. In choosing my subject, I have deviated somewhat from the usual practice heretofore followed, not because I have anything particularly new to say or any particularly startling facts to disclose, but rather for the reason that it seems desirable at this time to emphasize some of the things that appeal to us as possibly having a marked influence on the future development of botanical work. To one who is necessarily thrown in contact with the somewhat hurly-burly affairs of life, the old meaning of botanical work is gradually giving way to something else—something that reaches out into practical affairs and pushes its way into paths where, a few years ago, the botanist would have feared to tread.

Now the question arises, is botanical science to suffer by this movement, or is it, after the first preliminary efforts, to emerge rehabilitated, stronger and more vital than ever before? I have neither fear nor doubt as to the outcome, and believe that the spirit which has made us commercially a leader of nations will enable us to build up a science which neither time nor change can seriously affect. It hardly needs any extended statement to call to mind the rapid changes which have taken place in botanical work and botanical thought during the past twenty years, yet a critical study of these changes is, to me, one of the most hopeful signs that our progress has constantly been in the direction of a stronger place in the world's usefulness and a higher plane of scientific thought. Twenty

years ago the botany taught in our universities and colleges was of such a nature as to meet the general requirements of the time. It broadened out rapidly during the last ten years of this period, but it was still limited in large part to systematic studies, with some few attempts here and there to enter the field of morphology, physiology, and the kindred branches. Perhaps no one thing has given a greater stimulus to applied botany than the organization of the various State Experiment Stations, under what is known as the Hatch Act, which became a law in 1886 and went into active operation a year later. Under the broad authority given in this Act, establishing a Station in each State and Territory, opportunities were afforded for advanced studies of both plants and animals in their bearing on agricultural development, and as a result there was an extraordinary demand for men, which, even yet, it is impossible to meet.

Coincident with the establishment of the Experiment Stations came a broadening of the work of the National Department of Agriculture, thus creating the need for still more men trained in certain lines. At this time the era of specialization was scarcely upon us, but such was the demand for men and work that the stimulus to those engaged in special lines was great.

Of course, this country was not alone in the movement which has just been described, for in Europe, and particularly in France, there was experienced the same need for help in applied lines, and as a result extraordinary efforts were put forth by those in charge of chairs in the various institutions of learning to meet these demands. The happenings such as we are describing are met with frequently in the progress of the world, and are really the culmination of more or less subjective thought, which, when the proper moment arrives, breaks into force and makes itself

felt in an objective way. It is found, therefore, that while this work was making rapid strides, the demand was so great for immediate practical results that sufficient attention was not always given to that accuracy and precision of conclusion that the world's best thought demands. There was a proneness, in other words, to sacrifice accuracy to utility. Helmholtz, long ago, sounded a warning on this subject, when he said that 'Whoever in the pursuit of science seeks after immediate practical utility may generally rest assured that he will seek in vain.' On the other hand, there is a class of investigators, and their numbers are considerable, whose work, for the most part, is largely ahead of the practical side. Possibly, taking all of the work that has been done in this country, the need is not so much for more research, but for the practical application of the researches already made to the everyday affairs of life. In some branches this, of course, has not been the case, as is evidenced by what has been accomplished in a number of important fields during the past fifteen years.

Of the different branches of botanical science that have been applied to the betterment of man, physiology and pathology stand preeminently to the front. We cannot lay any great claim to much in the way of studies in the pure science of physiology, but the practical application of these studies to the affairs of life has been considerable.

In passing, I may be pardoned for emphasizing somewhat in detail a fact that seems to be little appreciated, and that is the great value and usefulness of the individual or organization that can bring to the attention of the people the results of scientific work in such a way that mankind as a whole is bettered, and the struggle for life is made less a burden. What value to the world is a scientific discovery unless it is clothed by some gen-

ius with a force that will bring its usefulness home to thousands, where before it would have been known to but a few of the elect? While willing to admit that America, for very good reasons, has not as yet been able to take front rank in the way of original discoveries, no one will deny the fact that our country has quickly turned to practical account discoveries of all kinds where there was promise of practical results. So that while in physiology, laboratory investigations have been pushed with vigor abroad, our efforts have been, in the past, mainly in the direction of broad field work, which has added materially to the wealth and power of the country. This is particularly the case with the work on legumes and the application of laboratory discoveries to the problems connected with nitrogen supply and the rotation of crops. The extended work of Laws and Gilbert, and other experimenters, has done much to emphasize the value of the broad application of laboratory research in this field. It sometimes happens in work of this kind that its application is of such a special nature as to preclude a proper appreciation of its value in a general way. Such, for example, is the work of Löew, who three years ago undertook a very special problem having to do with the handling of tobacco, and which, in two years, was practically finished, but so changed the aspect of the work that it opened great possibilities in building up an important industry and adding wealth to the country as well. The keen competition in tobacco growing, and the fact that the finest grades were, in large part, imported, made it very desirable and important that all available information in regard to the crop be secured. The chief problem upon which light was needed had to do with the fermentation of the leaf. Prior to Löew's work, it was generally held that fermentation was, in large part, due to bacteria, and that the difference in the

aromas of tobacco might, to a certain degree, be controlled through the action of these organisms. Löew's work showed that the fermentation of tobacco was due to enzymes. The enzymes causing the fermentation were isolated and methods for controlling them were pointed out. As a result of this work improved methods of handling the crop have been developed and new industries established. Such, for example, is the Sumatra tobacco industry developed in Connecticut, which owes its incentive to the advanced work of Löew, and which bids fair to add a great deal to the material wealth of the country.

Plant breeding is another branch of applied work closely related to physiology, which has made rapid advances during the past few years. It is true that plant breeding leads off into horticultural and other fields, but the advances that have been made in this field in recent years have had their inception largely in botanical studies. The work, as a whole, has had for its object the advancement of industrial pursuits, and has aided materially in adding to the wealth and progress of the country. It is true that in some cases applied work in this line has been pushed in advance of scientific research, but this has led to no serious results, for notwithstanding a lack of knowledge as to the full scientific significance of the various operations performed, the results have in most cases shown far-reaching intuitive knowledge on the part of those who have actually been engaged upon the various problems. What has been accomplished by Bailey, Webber, Waugh, Burbank, Hayes and others has shown great possibilities, and the improvement made in many crops will, no doubt, in time, prove of more value than even the present seems to indicate.

In no branch of botanical science have the advances in applied work been more pronounced than in pathology. Twenty

years ago plant pathology was practically unknown in this country. Little or no attempt had been made toward systematic work in this field, and what had been accomplished was largely in the direction of applying information secured as a result of investigations abroad. The first attempts in the study of pathological problems were naturally confined to questions having to do with parasites. The effects of parasitic enemies of plants were pronounced and gave opportunity for the most ready investigation. In looking back, therefore, on the early development of the work, it is not strange to find that investigations, for the most part, were in the direction of economic mycology, for it was largely a study of parasitic fungi in their relation to plant diseases. The important problems connected with the relation of the fungus to the host and host to fungus were, for the most part, overlooked. Pathology, therefore, had its inception largely in mycological investigations, which later developed into a study of the host itself. This naturally led into the field of plant physiology and developed slowly the important work of investigating plant environment and its relation to pathological phenomena. It was early seen that no sharp line of distinction could be drawn between any of these various branches, and for this reason it became important to push the investigations along several different lines. To the early workers in this field is due the credit of laying the foundation which paved the way for a full understanding of the broad problems elucidated later, and as a result the science itself has been established on a firm basis.

It is the practical application of this science, however, that has attracted such widespread attention everywhere, especially the work which has been done in this country and in France. Prior to 1885 very little was known in regard to the treatment

of plant diseases. The discovery of the efficacy of certain compounds in the treatment of crop diseases about this time led to a rapid awakening of the importance of the subject, and for the next few years there was a phenomenal advancement in the field treatment of plant maladies. Improvements in laboratory methods also did much to stimulate advanced work, and made possible lines of research which were not practicable before the discovery of such methods. What has been accomplished in this field alone has done much to encourage applied work and show the importance of such work as an aid to the advancement of pure science.

It has become the practice of late to ignore the important part that systematic botany has played in making known the practical value of plants to the human race. In the rage for special problems the fact is often overlooked that many of them owe their inception to prior efforts in taxonomic lines. It is hardly necessary or essential to go into details upon the bearing of systematic botany to applied work; but in passing, attention should be called to the great benefit that has come to the country as a whole through the important work on grasses, forestry and medicine. Some of the earliest work in economic lines in this country was based primarily on the systematic study of grasses, the object being to determine their agricultural value. The early investigations of Vasey did much to call attention to the value of applied botany, and there has been developed from this work very important and far reaching lines of research, such as are now being carried on by the U. S. Department of Agriculture and many of the experiment stations. This work, while having for its basis systematic studies, extends into broad fields of agronomy and other lines, such as have to do with the improvement of pastures or range lands, and many other similar lines. The

same is true of many of the important investigations that have been carried on in the matter of studying noxious plants, as, for example, weeds, etc.

The advanced forestry work of the present also owes its inception, primarily, to systematic studies which were begun years ago, and which are still continued in order to form an intelligent and rational basis for many of the advanced problems in this field.

In medicine, too, the study of systematic botany has played an important part. It was the general practice in the early days for physicians to be trained in botanical lines, and a great deal of our most important information has been brought out by the work of these same physicians. In fact, it has generally been considered necessary for physicians to be pretty thoroughly posted on botanical matters; hence the close relationship of botany to the practice of medicine has always been recognized. With systematic botany as a basis, the study of *materia medica* has advanced rapidly and has formed an important item in the development of our work. The differentiation of pharmacy from medicine has also led to further advancement in these lines, and has done much to advance the value of the investigations.

Probably in no other field of botanical science has the applied work been of more value to mankind than in bacteriology, surgery and sanitation. The systematic study of the causes of disease has led to most valuable results, and in nearly all of these investigations the inception of the work can be traced to one or more lines of botanical science. Such, in brief, have been some of the advances in applied botany in this country, and with this somewhat hasty sketch in mind, let us turn our attention to the future and consider what opportunities are before us, and along what lines our efforts should be put forth in order to achieve the highest and best results.

Attention has already been called to the importance and necessity of constantly keeping in mind the fact that in the application of science we cannot be too careful as to the foundation of our work. In the race for results we are too apt to lose sight of this fact, and in the end we find, too late, that our entire fabric has been built of straw, and tumbles to earth at the first gust of wind. It is necessary, therefore, in looking to the future development of applied work in this country, that we should turn our attention, not so much to the older men who are already in the field, but to the younger generation, who are still to come up; and the training they are getting, or are to get, in the various institutions of learning throughout the country. It is too true that many of our institutions of learning have been slow to recognize applied science; and even now, with all the demand for applied work, little or no effort is being made to put this work on the basis where it belongs. The training in applied lines at this time is meeting with much the same opposition that science itself did when first introduced into our colleges—especially science as taught by laboratory methods, rather than science as taught by handing down from year to year doubtful knowledge long stored in dusty tomes. There was a time, and not so far distant, either, when to be a student in a science course in some of our institutions required considerable moral stamina; but all this is changed with respect to science; yet there still lingers that inherent hostility to all things practical, as is most strikingly emphasized in institutions where applied work, such as agriculture, engineering, etc., is made a part of the regular course. With the great increase of wealth in this country and the commendable spirit being manifested in the endowment and establishment of institutions of learning, the fact must

not be lost sight of that there may be some danger, as has been pointed out, in building up an 'educated proletariat,' a class who, as specialists, will care more for getting their names attached to abstruse technical brochures than they will for a treatise that will enable some struggling mortal to make life less a burden. Some one has truly said that the danger from education is not so much from its quantity as from its character, so that it is the character of our training that should receive most careful, conscientious and considerate thought.

This leads us now to a consideration of the nature of the training our young men should receive in order to fit them more especially for the opening fields of labor in applied botany, and at the same time make good citizens of them, whether they go into the work in question or some other equally important. Pure science, of course, must form the groundwork for this training, but in addition to that there should be parallel with it, throughout the entire course, a rigid system of training in the application of science to the practical affairs of life. It is needless to say that we do not have anywhere in this country, at the present time, such a course of training in botany; and for this reason the men who go into this kind of work must receive their training, in large part, after the college doors close on them. I do not wish to be understood as implying that this state of affairs is due to our teachers, for most of them recognize the fact just mentioned and are doing everything in their power to overcome it. The trouble is with our system of education as a whole, but more directly the body politic, which has, ever since mind training began, given preference to the ornamental rather than the useful. Nothing has done so much to weaken this idea in the human mind as science itself, and nothing can so strengthen science in what it can further do in this direction as to

teach its broad practical application to the affairs of life. It would seem, therefore, that the time is ripe for some decided action leading to a clearer understanding as to the methods whereby the increasing demand for men trained in applied botanical work may be met. The National Government alone is spending close on to a million dollars a year in this work, and the demand for the right kind of men far exceeds the supply. In fact, the Government, through lack of properly trained men, has been forced to undertake the training itself, a course which would not be necessary if the proper cooperation could be secured from the colleges. Here is a subject which might very properly be taken up by this Association, and more especially this Section, as it is one in which most of us are either directly or indirectly interested. I have dwelt upon it somewhat in detail, as it has seemed to me the foundation upon which all other matters are built. With the men that we have and the men we can get, what then are some of the problems with which applied botany in the future can hope to deal?

With the opening of new territory during the past few years there has of course developed a need for still broader work, for we are now especially pressed for tropical investigations, which we are unable to meet through lack of equipment and lack of properly trained men. Moreover, another and equally important field has been opened through the rapid extension of our population into the arid and semiarid regions; and the demand from these people for light on many subjects, which we are ill prepared to give. It seems to me that everything points to the fact that the heavy demands for applied botanical work for the next fifty years will be mainly in the field of plant physiology and pathology. The two subjects are intimately connected, and while there will, of course, be many physi-

ological problems pure and simple, somewhere and at some time these problems will be found closely associated with pathological phenomena.

Reverting to our Western conditions, arid and semiarid, there are many questions which demand immediate attention and which have an important bearing on the future development of the country. Such, for example, are those which have to do with the water supply of plants and the bearing of water supply on plant production. Irrigation is now an important factor in our industrial and commercial development, and the problems associated with it must be reckoned with. In the past the work in this field has been mainly of an engineering nature, such as the question of securing water and bringing it as economically as possible to the plants. Now arise far more reaching questions, such as how to handle this water in a way to attain the desired maximum results with the least expenditure of time and money. Given water, soil rich in plant food and proper heat and light, the productive power of plants is great if the requisite knowledge is present as to how best to utilize what nature and art supply. Such problems as these must, for the most part, be worked out in the field, but the field must be made to take the part of a laboratory, for laboratory methods on an extensive scale must be employed.

What is the effect of varying quantities of water on the longevity of a plant; how is the production of fruit and foliage affected by the water supply; how far can time of ripening, color, keeping qualities, and resistance to diseases and insect attacks, be controlled through the ability to control the amount of water used? These problems, on their face, appear simple, but they are important ones and to throw light upon them there must be most careful studies in a number of fields. Chemistry will of

course enter into these studies, but it must be a living, vital chemistry, if I may use such a term, and not the mere question of ash determinations. Closely related to the problems involved in water supply are those which have to do with so-called alkali soils, and their effects on vegetation. A question of supreme importance to the development of our western country is to know more of the effects of various mineral salts, severally and combined, on plants. With such complicated problems as present themselves to the investigator in this field, it is not safe to base any conclusions on the knowledge of how plants behave in a laboratory, where the action of a single salt or simple combination of salts has been determined. The fact that individual plants show marked differences in their ability to resist the poisonous effects of alkali salts opens up an interesting field in the matter of plant selection and plant breeding. Wherever crops are grown in alkali soils, especially under irrigation, the power of certain of these plants to make better growth and give greater yields than their nearby neighbors has been noted.

Profiting by these facts, an important field opens in the matter of developing alkali resistant plants, having the power to give relatively large yields in the presence of an unusual amount of soluble salts in the soil. Some interesting suggestions have been made in this direction by the recent work of Kearney and Cameron, and the same investigators have also pointed out the great economic advantages that may result from the combination of two or more salts which, individually, may be dangerous, but when combined have the opposite effect on plant growth.

The nature of the problems here briefly reviewed shows the broad scope of physiological investigations, for they merge at various places into ecology, pathology, chemistry and physics. There is, further-

more, shown the futility of attempting to solve such problems along one line of cleavage, for it cannot be done with any degree of satisfaction.

Aside from the problems mentioned, the field for applied work in plant nutrition is large. The physiological rôle of mineral nutrients in plants is little understood, and the effects of mineral nutrients on growth, singly and combined, should be explained. The power to control profitable plant production through a better knowledge of plant foods is recognized, but there is yet much to do in the matter of making clear little known or obscure questions on this subject. In the problems connected with the acquisition of nitrogen, however, are to be found some of the most important practical questions in this field. The results already accomplished in this direction, through the use of proper nitrifying ferments, have not been as successful as was anticipated, but this does not indicate that future work may not be made more profitable. There is much to be done in the way of investigating the life history of bacteria inhabiting the root tubercles of legumes, for unless such questions are better understood it will not be practical to apply our knowledge in any far-reaching way. The time will doubtless come, however, when our knowledge of the nitrifying organisms will be sufficient to enable us to apply, in a much broader way, the use of pure cultures of such organisms in general field work. Already encouraging results have been obtained in this direction, and steps are being taken to extend the practical application of these results as rapidly as circumstances will warrant. The future success of this work will no doubt depend, in large measure, upon the ability to properly grow the nitrifying organisms in large quantities and at an expense which will not curtail their use; and then to be able to distribute the

organisms in such a way that the farmer himself may use them at little expense, but with sufficient profit to pay for his trouble. It will be seen, therefore, that while these may appear as simple problems when looking at them from the purely utilitarian view, there is much work to be done in the laboratory, under rigid scientific conditions, before satisfactory conclusions can be reached.

It is in connection with the problems bearing on plant breeding, and the selection of plants better adapted to meet the special requirements, that some of the broadest questions of applied botany can be brought to bear. While, as already explained, plant breeding is more or less of a composite science and, to a certain extent, an art, physiology is, after all, the basis for most of the work. There is much need for further research work in the field of reproduction and heredity, especially with a view to obtaining light on many practical questions which are bound to come up within the next few years if applied investigations are to have their proper place. Admitting the necessity of these, it would seem that some of the more practical problems that must be considered within the near future will have to do with obtaining light on such matters as the securing of plants adapted to particular purposes and to particular regions. As population increases and competition in all lines of agricultural production becomes keener, the need for securing plants better adapted to certain conditions and which can be produced at a minimum expense, will become greater and greater. In the South there is already felt the urgent need for improved kinds of cotton varieties that will give greater yields and finer staple, in order that cheap labor of foreign countries can be competed with. There is also a demand for improvement in other plants adapted to the South, which will en-

able the Southern agriculturist to more generally diversify crops.

We have been told at former meetings of this Association, by members of other Sections, that within a comparatively short time the United States will not be able to grow the amount of wheat, and possibly other cereals, needed for consumption. These statements are based on our present yields and the increasing demands of population. If the figures are true it would seem important, therefore, that attention be drawn to the securing of varieties of wheat better adapted to existing conditions and yielding larger quantities of grain. This is a perfectly legitimate field for applied botanical work, and what has been accomplished already indicates that much can be done in the direction of largely increasing the possibilities of this country in the matter of cereal production. What is true of cotton and cereals is also true of many other crops, so that it is unnecessary to go into detail as to what might be accomplished in the way of causing not only an increased output, but improving the quality of the output as well.

Associated with the work of plant breeding, and more or less closely related to it, is another important field which has for its object the studies of life histories of principal crop plants, with a view of determining the environmental conditions necessary for successful growth. This work, of course, covers a broad field, as it involves knowledge of the requirements of climate and soil, and really merges into the broader territory of ecological work. The problems involved carry with them, not only the question of plant adaptations, but the matter of introducing new plants from foreign countries and the broader dissemination of plants already existing here and which give promise of more profitable yields under changed conditions of environment.

With proper studies of soil and climate,

the possibility of more intelligently defining the areas adapted to certain crops will become greater. After all, however, the vital questions involved in this problem will depend largely upon actual experimentation, as those most familiar with successful crop production know how unsafe it is to generalize in such matters. The success or failure in growing a certain crop often depends on differences in soil and climate so slight that present instruments cannot determine them, although the plant, with its power to respond to unmeasurable stimuli, can do so.

In the field of pathology the opportunities for applied work in the near future will be great. We are all agreed that the more or less empirical methods of handling plant diseases has about reached an end. It served a useful purpose in pointing out practical ways of controlling some of the common and destructive plant maladies, and enabled those who were looking to the future to create a sentiment making possible better and more far-reaching work. We do not agree with those, however, who hold that the time is at hand when we can afford to stop the propaganda of actual field treatment. In fact, we are more and more convinced that one of the greatest opportunities for bringing home the practical value of pathological studies will be to undertake at once, on an extensive scale, what may be called demonstration experiments. A propaganda in this field, conducted by and depending upon publications alone, no matter how practical such publications are, will necessarily be slow; but when the work can be carried into the field and be made to serve as an object lesson, the impression made is lasting and convincing.

One of the problems, therefore, for the future, in this work, is how to insure the application of the investigations made and to so conduct the work that it will all go

toward the development of a system of plant pathology which will build up and strengthen the science. Recognizing the importance and necessity for the application of remedial measures in the form of fungicides, to which the foregoing remarks mainly apply, we may turn our attention from this art, for so it is, to other methods of applied work in this particular field of botany. The future of other lines of applied work all hinges on a recognition of the possibilities within the plant itself, its plasticity and ability to change, the effects of environment and the means of controlling environment or controlling the plant to meet the requirements of environment, to the end of securing desired results. Here again the breeding of plants will enter and furnish the means of overcoming diseases by selection of resistant varieties from those already existing and the creation of new varieties having the desired characteristics. Here, too, arises the question as to what factors govern resistance to disease, and how these factors may be determined and controlled. Why is it that the most successful production of a plant is often reached when its ability to resist the attacks of organisms or to succumb to functional disorders, is at a minimum, or, expressing it in a somewhat paradoxical way, why is a plant weakest when it is apparently most vigorous?

Proper knowledge on many of the problems involved in the questions here presented will make it possible to apply it in securing crops at far less risk than at present, and will tend to make the occupation of plant growing less a matter of guesswork than it is now. No rational system of pathology can be developed, furthermore, without due attention to proper field hygiene, the rotation of crops, and other similar means of surrounding the plants with healthful conditions. Some of the principal lines of work, therefore, in the

future, in this field, will be in the direction of giving a broader application to existing knowledge on the question of treating plant diseases by means of fungicides, to the development of new forms better able to resist diseases and suitable for special conditions, to the handling of plants so as to better adapt them to conditions at the present, and to the improvement of field methods to the end of securing vigorous growth by furnishing conditions needful to the highest production of the crop.

Of the future problems in other lines of applied botany, it is not necessary to speak in detail. Suffice it to say, that in the broad field of forestry, agrostology and pharmacology, systematic botany will always play an important part. In agrostology, especially, which has now come to be understood as covering the study of not only the true grasses, but all forage crops as well, the field for applied work is exceedingly broad. With the rapid settlement of the East and the utilization of our arable Western lands for crops, the areas for the maintenance of stock is becoming less and less. Thus is developed the necessity for a better understanding of methods of improving and maintaining our pastures. The production of larger quantities of forage from given areas and the improvement of our range lands to the end of enabling them to support an increasing number of cattle, are some of the other important problems in this field. These broad questions will, of course, involve to a certain extent systematic studies of native floras, the changes which may result from the shifting of plants from one place to another, and the opportunities that may arise from the introduction of new forms and the improvement of those already present.

Within the last few years it is fortunate that a well-defined forest policy has been developed, so that in the future the growth of this work will be largely in a distinct

field. Botanical investigations, however, will always play a more or less important part in all matters pertaining to the subject, especially systematic studies of the tree floras and the application of these studies to questions having to do with reforestation and the protection of existing forest areas. The applied botanical work, in connection with future problems in pharmacology, will be considerable. Systematic studies of plants used in pharmacy, the introduction and cultivation of such plants with a view to increasing their usefulness, all come within this scope of applied botanical research. The study of tropical plants, which has already been referred to, is also bound to play an important part in the near future in the matter of the development of our insular possessions. As yet, we have very little satisfactory information as to the possibilities of tropical agriculture, especially as concerns our own country; and it would seem that some of the first problems will have to do with systematic studies of the field to determine existing possibilities, with a view to applying them in the near future in a practical way. There are numerous practical questions having an important bearing on all tropical work, which must receive attention before any final conclusions can be reached in regard to the successful growing of crops in these regions. These questions have to do with the interrelation of the plants themselves to the development of the existing system of tropical agriculture, so that really a systematic study of our tropical floras would seem one of the first requisites offering a key to the future solution of other and more general problems.

Bacteriology, in its relation to surgery and sanitation, has passed out of the field of applied botany, but problems will still arise. Systematic studies of the bacteria may be essential to the successful prosecu-

tion of certain phases of this work. It is hardly necessary to refer to these questions in detail, and I may therefore conclude this somewhat hasty and general sketch of the possibilities of applied botanical work, as we see them, by again calling attention to a fact which becomes more and more evident as we look into work of this nature, and that is, how thoroughly we are all dependent on others for aid, not only in our own field of science, but other fields as well. Like our social fabric, science for science's sake and applied science are becoming more and more a delicately complicated system, capable of endless harmonious expansion if viewed aright, but leading to possible endless discord if handled wrong. How essential, therefore, that the broadest spirit of tolerance should be cultivated, for no matter how small or how humble a piece of real work is, somewhere and some time it may be made to form a part of an harmonious whole. While this is a practical age, and while the demand is heavy for practical results, we should not forget that there are ages to come after us—ages that may demand something different from what the majority of us are producing now; and for this reason the laborer in some obscure field should not be forgotten, for it perhaps may be that his work, now little known or understood, may in the future take its place in the building up of mankind.

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SCIENTIFIC BOOKS.

Histoire de l'Observatoire de Paris de sa Fondation à 1793. Par C. WOLF. Paris, Gauthier-Villars. 1902. Pp. xii+392; 16 plates.

If there had come down to us from the author of the *Almagest* a detailed account of the home of the Alexandrian school, the dimensions and cost of its buildings, their arrange-